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THE EGESTA DECREE *IG I<sup>3</sup> 11*

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The date of the Egesta decree is dependent upon the restoration of the archon's name in line 3, of which only the two final letters—omega, nu, of stoichoi 37 and 38—are beyond question. Contested letter-traces occupy stoichoi 35 and 36. Before stoichos 35 surface wear has erased any clues whatsoever to the archon's name.<sup>1</sup> There is unanimity that there are only two possible candidates: Habron (458/7) and Antiphon (418/7).<sup>2</sup> Plausible historical contexts have been presented for either date<sup>3</sup> and consequently the dating—and the potentially radical implications for our view of the development of the Athenian empire<sup>4</sup>—hinges on epigraphical considerations alone. According to Pritchett (*AJA* 59 [1955] 59) wear to the stone had "obliterated the name beyond recovery," but this view rested principally on his conviction that the vertical trace in stoichos 35 is a scratch rather than a mason's stroke. It will be argued that there is good reason to reject this conclusion.

The surviving text contains three-barred sigma exclusively but ten tailed rhos and two tailless rhos (Chambers et al. [above, n. 1] 41). It has been held as axiomatic that three-barred sigma was not used in Attic public inscriptions after 446 and that tailed rho was not used after 438/7 (R. Meiggs, *JHS* 86 [1966] 93). As our document uses both tailed and tailless rho this latter epigraphical "procedural rule" must be disregarded here. Meiggs' *terminus post quem* for three-barred sigma rests upon the absence of this letter-form in any (then) securely dated decree after that time; but conversely it has been appealed to since to insist on the pre-445 dating of all Attic public inscriptions containing three-barred sigma (noted by Chambers et al. [n. 1] 55).

Chambers has reasonably held (*ibid.* 57) that because the quantity of extant epigraphic evidence is still relatively slight the three-barred sigma dogma should not be regarded as prohibitive of potentially later datings. Because consent over the reading of the archon's name is far from unanimous and because if our decree does belong to 418/7 it will disprove the earlier *terminus post quem*, that dogma must be suspended in any examination of the decree.

Recent attention has focused upon claims by Chambers that three new investigative approaches—letter measurement, photographic enhancement, and laser photography—can positively resolve the dispute over letter-traces in stoichoi 35 and 36 in favour of Antiphon. Chambers first argued, in support of iota in stoichos 35, that "measurements of the space between letters within a given line" based on groups of four letters beginning with "a clear iota" and similarly on groups of four letters beginning with "a broad letter" could lend determinative weight to the reading of the contested letters.<sup>5</sup> Against this, Henry stated that "although ... on balance the combinations of letters of the one group occupy more space than the combinations of the other, there is in fact little difference between certain combi-

<sup>1</sup> A.S. Henry, *ZPE* 91 (1992) 138, confirms that nothing can be read in or before stoichos 34 either on the stone or in squeeze evidence. B.D. Meritt, *PAPhA* 121 (1977) 444 and n. 22 with fig. 6, conceded this for the stone but claimed that despite the washing of a museum numeral from the surface, paint had left traces of "deeper color" in what he took "to be the bottom part of the left stroke of alpha in the upper curve of the middle 6 of 266 and a somewhat darker shading (barely perceptible) also where the right stroke of alpha must have been." I can find no sign of any such left trace in his photograph. The accompanying claim for a right trace is wrong—the bottom right of the stoichos is well clear of any of the painted numerals, as is readily apparent in his figs. 2 and 3, and must lead the whole claim to be rejected. M. Chambers and E. Erxleben reexamined squeeze evidence cited by Meritt but could find no trace of any part of a letter in stoichos 34—see M.H. Chambers, R. Gallucci, and P. Spanos, *ZPE* 83 (1990) 39.

<sup>2</sup> Meritt (1977) [above, n. 1] 437; M.H. Chambers, *CJ* 88 (1992/3) 26.

<sup>3</sup> For Habron, R. Meiggs and D.M. Lewis, *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B.C.* (Oxford 1988) 81; for Antiphon, H.B. Mattingly, *Historia* 12 (1963) 267-268, 270.

<sup>4</sup> Outlined by H.B. Mattingly, *Historia* 41 (1992) 129-138 and citing previous work.

<sup>5</sup> Chambers et al. (n. 1) 41 and Appendix II; and again in M.H. Chambers, *ZPE* 98 (1993) 173.

nations in the two groups;" that "the stone is not cut with complete regularity;" and that the measurements do not compel a reading of iota over beta in stoichos 35 (above n. 1, 141-42).

In a restatement of his position, Chambers argued that it was not important that the letters had precise vertical alignment but that "the critical factor ... is the space allocated [to each group of letters] horizontally, that is, within a given line" (1992/3 [above, n. 2] 30). Henry has since re-asserted (*CQ* n.s. 45 [1995] 239) that "vagaries in vertical alignment must of necessity create irregularities of *horizontal* spacing" and that because three groups of letters on the preserved surface do not conform to the norms of Chambers' measurements that study has failed to produce compelling evidence for either beta or iota. Against this, Chambers holds that because Henry has not shown "a group of four letters beginning with a 'broad' letter that matches the smaller space occupied by our 'iota plus three' in IΦON," he has not successfully countered Chambers' argument based on measurements.<sup>6</sup> In summary, it seems that the evidence, while favouring iota over beta on the basis of letter-group measurements, has not proved compelling for all scholars because of irregularities in letter spacing and severe surface wear to this text. I therefore rely on other considerations for the argument advanced in this paper.

Chambers' second claim was that photographic enhancement could shed light on the identity of both disputed letters. He and his colleagues published plates from which they read iota and phi.<sup>7</sup> This reading was disputed by Henry, who observed that "what the camera records and the image processor refines is simply what is now 'visible'; ... we are now driven to making ... subjective assessments of the enhanced images the computer has produced for us" (1992 [above, n. 1] 142). That is, the enhanced photographs are not *in themselves* more compelling than previous unenhanced images or the accumulated testimony of disputed observations of the stone

Lastly, Chambers claimed that a laser beam directed through the stone showed "a clearly rounded shape in stoichos 36 that could not be rho and can be interpreted as phi", and that this was readily verifiable from his accompanying plates.<sup>8</sup> Yet this claim is subject to question due to the potential of the technique to "reveal" as markings what may in fact be sub-surface flaws and also to the element of subjectivity still required in interpretation of the resultant laser-generated photographic images, as Henry has stated (1992 [above, n. 1] 145). The plates do not compel a reading of phi in stoichos 36, which has emerged as the current *metaichmion* of this dispute. One can only conclude that the evidence here, while telling strongly against rho, cannot be made to positively support phi, and that consequently there are no grounds for reading any dotted letter in this stoichos.

Concentration on this letter-trace has shifted attention away from stoichos 35; but I suggest that the present stalemate may in fact be objectively resolved by a renewed consideration of the accumulated evidence concerning that vertical marking. It is most important to determine what this trace is or is not: should it be iota, Antiphon is guaranteed (1992/3 [above, n. 2] 27); conversely, beta guarantees Habron, irrespective of the ability to certainly determine the subsequent letter in stoichos 36.<sup>9</sup> Pritchett thought it "deeper than the incised letters and surely a scratch" (*AJA* 59 [1955] 59) but although the surface is worn in the neighbouring stoichoi, one can see verticals of equal depth in the rho and final epsilon of EPXE following.<sup>10</sup> The depth of the vertical therefore gives no reason to doubt that it is a deliberate ma-

<sup>6</sup> M.H. Chambers, in correspondence of 3 March 1995; cf Chambers (1993) [above, n. 5] 173.

<sup>7</sup> Chambers et al. (1990 [above, n. 1]), color plates A and B; the enhanced photographs are also reproduced in black and white in Chambers (1992/3 [above, n. 2]), plate 1.

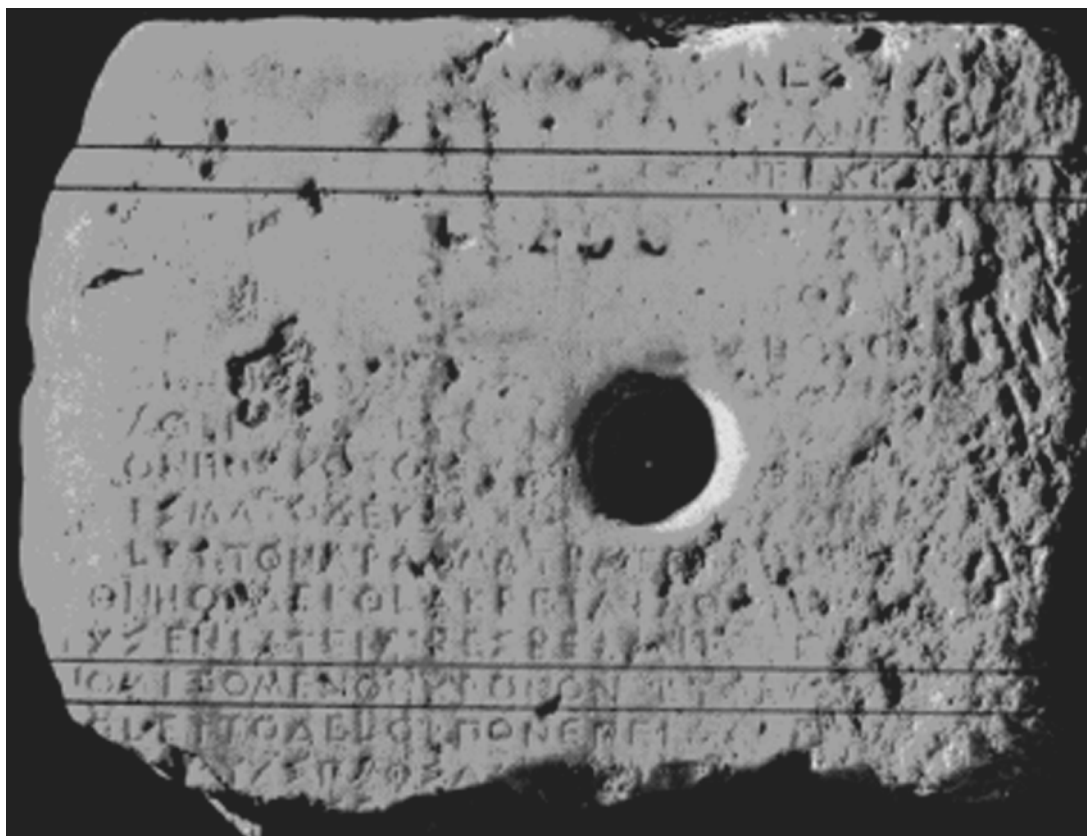
<sup>8</sup> Chambers et al. (1990 [above, n. 1]) 43 and plates II and III; again, in Chambers (1993 [above, n. 5]) 171, he held that the plates "speak for themselves" in showing iota, phi from Antiphon.

<sup>9</sup> Epameion (429/8) is ruled out by the rounded traces in stoichos 36 which are incompatible with nu—Mattingly (1963 [above, n. 3]) 269.

<sup>10</sup> H.B. Mattingly inspected the stone again in January 1995 with this question in mind, and wrote, in correspondence of 22 March 1995, that our vertical appeared to be "no deeper cut than the two verticals [here] instanced—indeed no different from many other vertical strokes in the rest of the text."

son's stroke. Scrutiny of photographic evidence additionally indicates that the vertical is a cutting of even width from top to bottom.<sup>11</sup>

Chambers wrote that the stroke "is of precisely the same length as the other clear iotas on the stone" (Chambers et al., 1990 [above, n. 1] 43). While one must agree with Henry that "precisely" is an overstatement given "the difficulty of precise measurements of letters at any point on this extremely worn surface" (1992 [above, n. 1] 143), it is nevertheless clear that our vertical is of a length consistent with many other inscribed verticals. It has been held that it seems to extend too high to be a letter (*ibid.*) and this in fact seems the principal reason for some refusing to accept the mark as a mason's stroke. However, the stroke is centered vertically within its stoichos. To verify this, I drew on a copy of the text a pair of parallel horizontal lines, between lines 14 and 15 and between lines 15 and 16, where the horizontal alignment of the letters is clear. I then drew further lines parallel with these, between lines 2 and 3 and between lines 3 and 4 of our document (see scanned photograph, below). The position of the letters on the vertical plane is revealed by their placement between these horizontal lines. The trace in stoichos 35 is equidistant from these lines and is clearly consonant with the alignment of the other remaining letters in line 3. One must therefore concur with T.E. Wick that the mark "is of proper height, direction and position to be a normal vertical stroke of a letter" (*JHS* 95 [1975] 187).



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(scanned from D.W. Bradeen and M.F. McGregor, *Studies in Fifth Century Attic Epigraphy* (Oklahoma 1973) 73, pl. VII); the horizontal lines were added as described above.

<sup>11</sup> Chambers et al. (1990 [above, n. 1]), color plates B1 and 2. At first glance one may gain an impression that the vertical stroke seems to taper downwards; but the bottom of the stroke may be seen to extend below the paper fold which cuts across *ZPE*'s color plate B2, and to be of the same width as the more distinct upper portion of the stroke.

There are, in Chambers' words, "a couple of small diagonal marks, slanting down to the right, near the top of the vertical," (Chambers et al. [above n. 1] 43), and a further problem has been stated by Henry: how are we "to differentiate between the vertical stroke and the other traces descending to the right from the top of this stroke? Why is the former to be retained as a genuine letter trace and the latter to be disregarded?" (*CQ* n.s. 45 [1995] 238).

Meritt held that these marks formed the upper half of an angular beta, a letter-form which Meiggs could not observe on this stone in his seminal article on epigraphical dating.<sup>12</sup> Chambers wrote that the marks are "the result of damage to the stone and are not part of the enclosed sections of a beta: the only preserved betas ... are carved with rounded loops, with which these small diagonal marks are incompatible" (Chambers et al. [above, n. 1] 43-44). In an examination of the stone in the wake of Wick, Balcer stated that the marks next to the vertical are "clearly scratches" and that "Meritt's beta will not hold."<sup>13</sup> Bradeen and McGregor observed that the marking here is "very light, ... a mere scratch,"<sup>14</sup> which does not support the existence of any chiseled stroke even given surface deterioration. Scrutiny of the photographic evidence shows that the markings join neither each other nor the vertical but comprise two small and distinctly separate pittings which are not consistent with a continuous chiseled cutting;<sup>15</sup> nor does their shallowness allow that they are residual pits left by a chisel after a surface channel was inscribed. Although there may be some subjectivity involved in "mark" versus "scratch", it is evident that there is good reason to deny that these marks are associated with our vertical. Certainly we do not have an angular beta; and neither are the marks consistent with the rounded lobes of the other two betas on the stone.

It may be concluded at this point that it is not possible for proponents of Habron to confirm a reading of beta, and that in reading Habron both beta and rho—that is, all of the evidence for reading Habron—must be supplied. Furthermore, if all that I have said concerning the vertical stroke is correct, then the accumulated testimony requires that it be acknowledged as iota. Because the subsequent letter in stoichos 36 cannot be determined, the stone must be read: -II[.]ON (restored, ANT)II[Φ]ON), and accordingly dated to 418/7.

It has been objected that Thucydides does not mention any recent alliance between Athens and Egesta in his discussion of the Egestaian embassy of 416/5, and therefore the Egesta decree could not belong to 418/7 as such a recent exchange of oaths would not have been overlooked in the account; in other words, the Egestaians would have appealed directly to such an alliance and not to some more distant arrangement.<sup>16</sup> However, Thucydides states that although the Athenians aimed to conquer Sicily, they wished to make it appear as though they were sending help to their kinsmen and newly acquired allies (6.6.1). We should then see Egesta as among these (unspecified) recent allies on the basis of the Egesta decree, and indeed at 6.6.2-3 "it is clear that the offer of money specifically comes from [the Egestaians] as allies" of Athens (H.B. Mattingly, by letter of 12 May, 1995).

S. blower (*Thucydides* [London 1987] 91) observed that Thucydides often does not tell us what we otherwise know from inscriptions; it is we who think that he should have mentioned the historical facts which known inscriptions imply. Here, the Egestaians are appealing to parallel circumstances, not (or not as recorded) to a particular treaty. In 427 the Athenians had had designs on Sicily, as well as an in

<sup>12</sup> Meritt (1977) 443 with figs. 3 and 6; he has drawn in an angular beta on his Fig. 2. Meiggs, *JHS* 86 (1966) 94, Table 2; our text is no. 15.

<sup>13</sup> J.M. Balcer, in a letter cited by T.E. Wick, *CP* 76 (1981) 121.

<sup>14</sup> D.W. Bradeen and M.F. McGregor, *Studies in Fifth-Century Attic Epigraphy* (Oklahoma 1973) 76.

<sup>15</sup> Most clearly in Chambers' black and white photograph, (1992/3) plate 1; but the marks also show as distinct pittings in the unenhanced plates of Chambers et al. (1990 [above, n. 1]), color plates A1 and B1, and in the latex squeeze of Pritchett (*AJA* 59] 1955), plate 33A.

<sup>16</sup> E.g. by Meiggs and Lewis, *GHI* 81. As Chambers (1993 [above, n. 5]) 174, observed, those who would date the Egesta decree to the year of Habron also need to explain why the Egestaians do not appeal to an alliance document.

terest in preventing grain shipments to the Peloponnese, and had sent a fleet to aid Leontini on the pretext of kinship (3.86.3). Thucydides' point is the duplicitous nature of Athenian assistance, in both cases masking its Western ambitions behind its ostensible "obligations." The Egestaians presumably knew of the Athenian ambitions as they spent some time in Athens endeavouring to persuade the Athenians to render aid (6.6.3). They therefore remind the Athenians (6.6.2) of the "fighting-on-the-same-side which occurred" in the time of Laches,<sup>17</sup> that is, of the Athenian interests and plans which had underpinned their sending of a fleet to the aid of Leontini. They do so because they are well aware of the difficulty and expense of sending a fleet such a distance.

Because the Egestaians appeal to parallel circumstances (of only a decade earlier), rather than to the text of a diplomatic instrument or alliance, the grounds for the above objection do not hold, and the Egesta decree may be seen to be not at all incompatible with Thucydides' account. Chambers noted that to date the Egesta decree to 418/7 fits well with the historical context "between Athens' first forward moves in Sicily in 427 (Thuc. 3.86 and 88) and the tragic decision to intervene in Syracuse," and Smart earlier argued that a dating of 418/7 "would fit admirably into what we know of the internal political situation at Athens between 421 and 415."<sup>18</sup> The late dating is therefore consistent both with the narrative in Thucydides 6.6 and with the known development of Athenian foreign policy.

While one might (rightly) continue to assert, with Henry, that absolutely *incontestable* proof of a text employing three-barred sigma after 446 has yet to be unearthed, I submit that on all criteria the evidence indicates that our vertical is iota. Consequently I suggest that there is sufficient justification for a full exposition of an "alternative model" of the Athenian empire which takes into account the downdating past 446 of texts employing three-barred sigma in accordance with non-epigraphical historical criteria.<sup>19</sup>

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<sup>17</sup> So Dover, in A.W. Gomme et al., *A Historical Commentary on Thucydides IV* (Oxford 1970) 221.

<sup>18</sup> Chambers et al. (1990) [above, n. 1] 55; J.D. Smart, *JHS* 92 (1972) 138.

<sup>19</sup> I wish to thank Alan Henry, Mortimer Chambers, and Harold Mattingly for their detailed criticism of successive drafts of this paper, and the Epigraphical Museum, Athens, for permission to reproduce the photograph above.